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Interview Jacob Murre

Remembering Grothendieck

On 13 November 2014 Alexander Grothendieck, one of the greatest mathematicians of the twentieth century, passed away. In memory of Grothendieck, Ulf Persson interviewed Jacob Murre, professor emeritus of Leiden University. This interview has been published in *EMS Newsletter*, Issue 96, June 2015, and is reprinted here with permission.

You may be among the still living people the one who has known Grothendieck for the longest, in fact you were almost exact contemporaries. When was the first time you met him?

“It was in the spring of 1955 at Chicago. As to the first remark this cannot be the case. Serre, Ribenboim and Cartier definitely met him earlier, and undoubtedly they know him much better.”

Anyway this is a pretty exclusive set. What were you doing in Chicago? Were you a post-doc?

“No, I was still a graduate student, but my advisor Kloosterman had sent me to Weil in Chicago to learn algebraic geometry.”

So what was Grothendieck doing there?

“He was actually at Kansas at the time doing functional analysis, or maybe he had already moved to homological algebra. Weil had invited him to give a lecture because he had already acquired quite a reputation as an upcoming bright mathematician.”

So what was your first impression? How did he appear? Had he already shaved his head?

“To disappoint you I do not remember much of his visit. I went to his lecture, which was on functional analysis. I had at the time naturally no inkling that he would become one of the very greatest mathematicians of the twentieth century. As to his appearance I have no recollection, but if it would have been extreme in any way, I certainly would have remembered.”

When was the next time?

“That was at the ICM in 1958 in Edinburgh where he gave a famous lecture outlining his visions of the development of algebraic geometry. Unlike the first time this made a really deep impression on me. I even was able to ask him some questions later during the congress. But our discussions were of necessity rather superficial, he was at the center of attention, always surrounded by people. He did give me a preprint though (written by Borel and Serre) on his work on the Riemann–Roch theorem.”

And this was when your relationship started in earnest?

“I would say that happened the following year when he wrote me to ask whether I could generalize my key theorem of my

PhD thesis also to hold in mixed characteristic.”

By the way what is the key theorem about?

“It is the so-called ‘linear connectedness theorem’ saying that the total transform of a smooth point by a birational transformation is linearly connected, meaning that any two points can be joined by a sequence of rational curves inside the transform. However, I was only able to prove this over a field.”

So you mostly related via letters?

“In fact that same year Nico Kuiper, later to become director of the IHÉS but who at the time was professor at the Agricultural University in Wageningen, invited him to give a lecture there. I attended the lecture of course, and afterwards Kuiper took us to his house, and then I finally got an opportunity to speak extensively with him.”

What did you talk about?

“The Picard variety, which at the time was very much at the center of interest. Matsusaka, Weil and Chow had already constructed it algebraically, but Igusa had discovered pathologies in positive characteristic. They were very mysterious. Grothendieck knew it all of course and I asked him whether his new theory of schemes would be able to explain and even remove those pathologies. Grothendieck told me that he had not yet given those matters

serious thought because the theory would be treated in chapter XII in his forthcoming EGA.”

This is a remarkable statement. He was really planning ahead, and it also speaks great confidence in his powers.

“Yes, he was very confident that he would clarify the questions when he would get around to it. Not only that he claimed that the people just mentioned made too strong assumptions and tried to prove too little. He would make less assumptions and prove more.”

You must have been very impressed, or did you think he was merely bragging?

“Let me say that my attitude was one of skepticism. At the time I wisely said very little.”

But he was not bragging?

“Of course not. He did eventually fulfill his promise three years later, if not actually in chapter XII of EGA but instead in his two beautiful Bourbaki lectures (232 and 236) where he constructed the Picard Scheme and thereby explained and removed all the pathologies.”

You must have been impressed?

“Very much so.”

But let us backtrack. You had been brought up in Weil's foundations, what was your attitude to schemes initially?

“I certainly had made much of an effort to learn the language of Weil and thus I was naturally very hesitant to jettison all that effort in order to acquire yet another language. But I think the word ‘language’ is misleading, although I know it is often used in this context, I would prefer the word ‘theory’. In the end I decided to ask the advice of Weil. I trusted him very much and was convinced that he would give me the right advice. By that time Weil had already left for IAS at Princeton, and in the spring of 1960 I was at Evanston and I made a visit to the Institute.”

Weil has the reputation of being a rather nasty man and many people admitted that they were afraid of him. I have also heard that Weil was rather jealous of Grothendieck and his advances in algebraic geometry feeling dethroned. It must have been a very sensitive subject to bring up with him.

“First let me point out that Weil has always been very kind to me, and I am and will always be very thankful for all the things I have learned from him...”

... That makes perfect sense. If a great mathematician is ‘nasty’ it is because he cannot suffer fools gladly...

“... Let me finish. I visited Weil and we took a walk in the surrounding woods, which all visitors to the institute are very familiar with. I then brought the matter up with him.”

What did he say?

“He said ‘Grothendieck is very strong. He has done things, nobody of us has been able to do.’”

Whom did he refer to specifically?

“I did not dare to press him on that point. He had made his point. The master had spoken and the message was not only clear, but as it would turn out very great for me. So from then on I started to study schemes.”

So when did your collaboration with Grothendieck start?

“I would not call it a collaboration it was not that close, but I think that it was in 1961 when he invited me to IHÉS, and I went there in the first half of 1962.”

So you went to Bures-sur-Yvette?

“No, at the time the famous SGA seminars were in fact still held in Paris, in the 16th arrondissement, by the way in a building of the Fondation Thiers. But I lived out in Bures in one of the apartments the Institute had acquired.”

Just to get the flavor, could you describe the scene?

“I will do my best. It was always held on a Tuesday afternoon. Arriving before the lecture I would typically find Grothendieck and Serre engaged in a lively discussion. Dieudonné was there of course, and during my term Néron was a visitor too. Then of course there were all the students of Grothendieck.”

Who were they at the time?

“I do not recall all the names, but certainly Demazure, Gabriel, Verdier, Raynaud along with Mme Raynaud. But you can

see that Grothendieck was always busy, so much demand on his time, so there were few opportunities for me to speak to him.”

Are there any other things you remember? Did Grothendieck do all the lecturing?

“No. During the first few weeks Néron gave a series of lectures on his theory of the Néron model following upon Grothendieck’s regular lectures. They were, however, phrased in the language of Weil and I suspect that hence they were a bit difficult for most of the audience to follow. As to other things, I should not forget Mlle Rolland, the secretary, who saw to all the practical things and made it all run so smoothly.”

So nevertheless you had few opportunities to talk to Grothendieck?

“At the seminars, yes, I had few opportunities, but Grothendieck also invited me to his home. At the time he was living with his family in Paris, on the Île de Jatte to be precise.”

This sounds exciting, could you please tell us what was going on?

“Luc Illusie has described them beautifully in his note ‘Reminiscences of Grothendieck and his school’, his experiences being very similar to my own. But to be more specific as to my own, I would arrive after lunch and be alone with him. Naturally I took advantage of the opportunities and asked him a lot of questions, no doubt very simple ones, maybe even occasionally stupid ones, but no matter what he would always be very patient and explain carefully, even what to him must have seemed very elementary ones.”

Illusie has also told me of this experience with Grothendieck. He was never at a loss for an answer I take it.

“Not always, sometimes if very rarely, he did not know of an answer.”

What would happen then?

“He would say something to the effect that he thought that he had considered the problem, then he would turn around and open a cabinet just behind his chair. The cabinet would be crammed with handwritten manuscripts and he would take one out, glance at it, and then come up with an answer.”

What kind of questions did you ask him about?

“As you surely know the written final versions of his work are so general and overwhelming, I would even say intimidating, so mostly I asked for clarifications.”

And he was able to give those, without intimidation?

“Yes, very much so, because when you discussed with him privately it was so different. He always took as a starting point a natural problem in order to relate it to his ideas, which consequently became so much more understandable.”

Nothing beats a personal discussion to convey mathematics.

“That is very true. To hear him explain his marvelous ideas or to see how his brilliant mind attacked problems, are what I treasure most among my mathematical recollections.”

So you would have him all to yourselves during those afternoons?

“You make it sound as if it would have been a very regular occurrence, in fact it did not happen that often, but often enough. And typically after our afternoon session, he or his wife Mireille would always ask me to stay for dinner. They were very hospitable. Invariably after dinner Grothendieck would resume expounding on his ideas, and often I got so engrossed that I had to hurry to catch the last train out to Bures.”

So your switch from varieties to schemes turned out to be a wise investment?

“Very much so. For my generation it was a revolution. In fact during my first visit to his place I asked him why he had come with the notion of a scheme, when varieties constituted, and still do of course, such a beautiful subject with lots of deep theorems and challenging problems.”

And what did he say?

“Basically he claimed that nilpotent elements exist in algebraic geometry by nature. To neglect them, i.e. to remove them, is an artificial, not to say a brutal form of surgery, akin to amputation. They are there for a good reason, to ignore them lead only to confusion, even to pathologies. By taking them into account not only will we rid ourselves of pathologies we will also understand varieties better and get new

powerful tools to attack classical problems involving varieties.”

And what did you think of that?

“It opened my eyes. Contrary to what many may think, Grothendieck did not develop the theory of schemes just for the sake of generalization but the reason was, or at least one of the main reasons, that you needed schemes to understand varieties.”

And you agree?

“Of course. To give just one example. The pathologies of the Picard variety in positive characteristics appear because you should really consider the Picard scheme. Technically, a scheme is needed to represent the Picard functor. And besides the power of the nilpotent elements is shown in his attack on the fundamental group of a curve in positive characteristics by lifting the curve to characteristic zero. I cannot emphasize enough that in Grothendieck’s approach to mathematics he was never striving for generalizations for its own sake.”

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Although this is a natural conclusion when you encounter his written work.

“Yes, maybe, but the key concept is not generalization but naturalness. He was always looking for the natural context, and with his fabulous insight and intuition he was almost always able to find this context, which, however, I must admit with some regrets, required generalizations.”

So those were forced upon us?

“Very much so.”

So this is a faithful summary of his philosophy?

“Very much so. Whenever he explained something to me, I could always sense this underlying strategy of his. By the way I would like to return to my pet topic of the Picard functor.”

By all means.

“As I have already referred to, during that

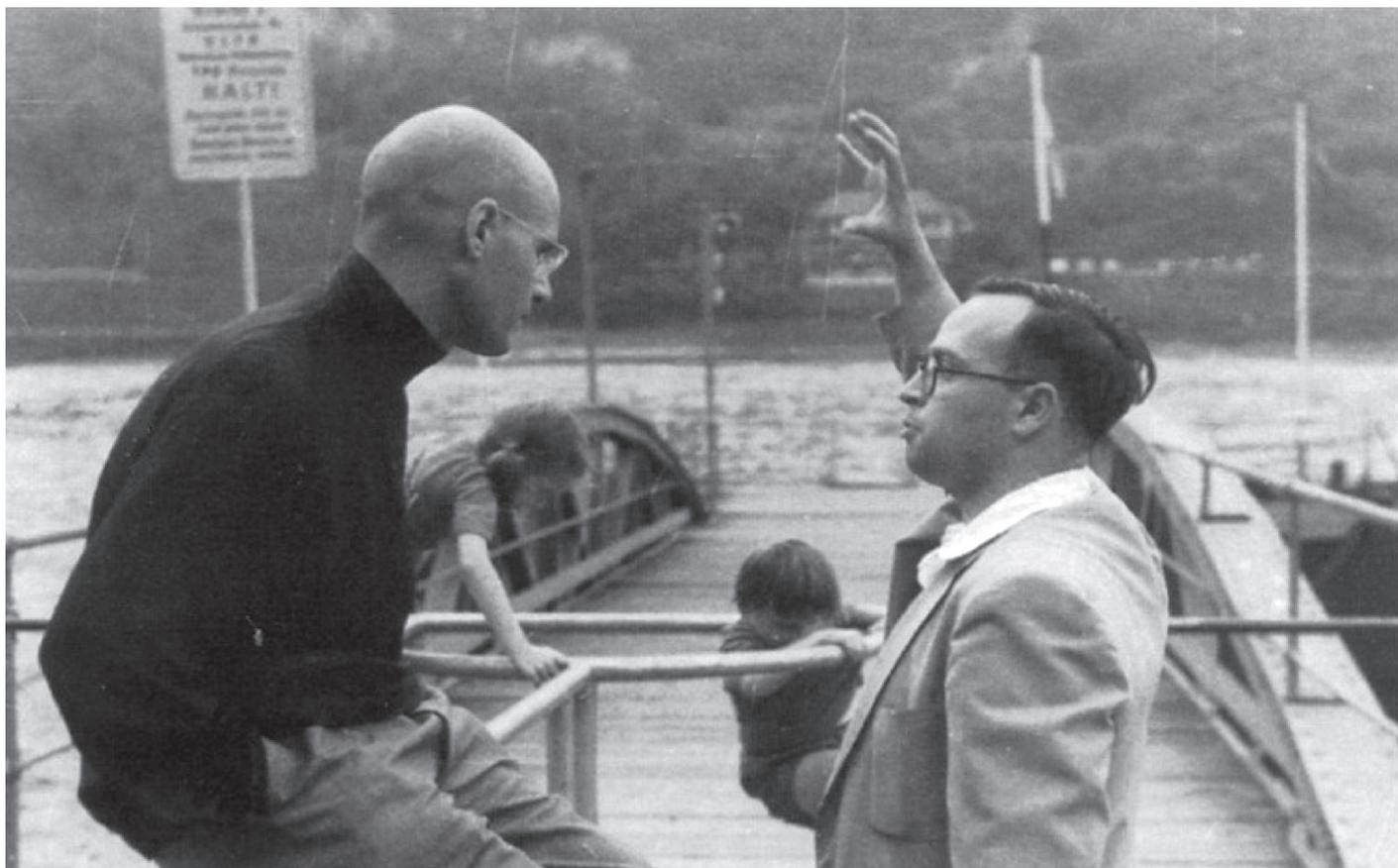
term I attended his two Bourbaki lectures on the Picard functor. In his construction of the Picard scheme he followed more or less Matsusaka’s original construction of the Picard variety, with the crucial exception of replacing Chow points with the Hilbert scheme. This relies heavily on projective methods and thus the case of a proper variety over a field was not covered. This gave me the rare opportunity to explain something to *him* instead, which needless to say, made me very happy. Between his two lectures I told him about the construction of the Picard scheme in this particular case. Of course I would never have been able to produce this construction had I not been properly instructed by him. After a long struggle I had finally understood his results on pro-representability of functors, and the existence and comparison results of EGA III furnished me with a powerful tool to enable me — at least over a field — to characterize functors representable by a commutative group scheme from which my insight on the Picard functor in the proper case dropped.”

This must have been a very satisfying experience to you. How did Grothendieck react?

“He saw immediately that it was all correct and during our subsequent discussion he even suggested some simplifications which I later incorporated in my paper. I should add though that my results were subsequently surpassed by the work of Mike Artin on representability of algebraic spaces.”

So by that time your collaborations with Grothendieck would start in earnest and continue throughout most of the sixties? I take it that you were a regular visitor to IHÉS.

“As I have pointed out before, ‘collaboration’ is too presumptuous a word to indicate my relation to Grothendieck. As to my visits to IHÉS during the year, they were indeed several, but because of my duties at my home institution at Leiden, I was normally only able to visit for a few days, with two exceptions. In 1963 I was able to stay for a month and in 1967 for a couple of weeks. Those visits were also somewhat different as IHÉS had definitely moved to Bures in 1963, and so had Grothendieck with his family, and later he would move on to Massy.”



Alexander Grothendieck (left) with Michael Atiyah, around 1961

Photo: courtesy of the estate of Friedrich Hirzebruch

Do you have some poignant recollections from that period.

"I have at least some that stand out. In particular, back in 1963, when I was on the train with him to attend a lecture by Hyman Bass to be given in Paris. We started to talk about what we would do when we were old. Grothendieck expressed a wish to become like Zariski, meaning following and enjoying the work of his former students."

But it would not turn out like that.

"No sadly not."

Anything mathematically that stand out?

"It would be his incipient theory of 'motives'. The first time I heard about it was in the fall of 1964, when I made a visit in preparation for a Bourbaki talk I was to present in the following spring. During a break in our intense discussions I asked him what he was working on at the time. He disclosed that he was working on a new theory, a theory he referred to as of 'motives', which would finally explain the similarity of all cohomology theories, and elaborated a little on his ideas. Later on

in 1967 he gave a series of lectures on his theory, but unfortunately I was unable to attend them, but were later informed about them by Manin, who had been in attendance."

You keep telling me that you did not collaborate with Grothendieck, yet there is a joint paper, not to say a monograph, with him.

"Let me put it this way. Grothendieck was always very generous in sharing his ideas. The paper to which you are referring started like this. Grothendieck and I took a walk together, I am not so sure of when, most likely in 1968 or 69. He told me that he wanted to study the tame fundamental group of a normal point on a two-dimensional scheme, in a way similar to Mumford's classical study. He already had an idea of how to do this and had in effect solved the major part of the problem, however, there remained some technical parts he had not yet resolved. He suggested that I look into it, as he had more pressing things to attend to. On my return back to Leiden I struggled with them, and after some time I was able to sort out the re-

maining parts, and of course I wrote him. He suggested I should publish those results on my own. I protested in my next letter, pointing out that the idea, as well as a large part of the solution was due to him. The only honorable thing would be to write a joint paper, and he agreed."

We are now approaching the end of the 60's and with that the end of the Grothendieck era. Can you report on its twilight?

"I would not use that word. It indicates a decline that was not present."

But you could perhaps see signs?

"Signs are often more pronounced afterwards than at the time when you have no idea of what they may portend. To give an example. The last time I visited Grothendieck at his home was in 1969. He had by then moved to Massy. Formerly he had never complained about his tasks and duties, but this time he admitted that writing EGA and taking care of SGA took a lot of his time. As I usually did visiting him I asked him about an update of the status of the Weil conjecture. He said that he would not

be surprised if one of these young people would come up with a solution..."

... did he mention any names?

"He mentioned Deligne and Bombieri. He thought so because he suspected that only one new idea would be needed to overcome the present deadlock."

And he was right?

"As usual he was, although the new idea was far from what he had hoped for and expected."

And on this we need not dwell. Was this also the last time you met him?

"No, not quite. I remember how in the evening of that final visit he walked me back to the station, barefoot. I was staying at IHÉS as usual. Actually the last time I met

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him in the flesh was the following year, at the ICM at Nice. By that time there was a definite difference from before. His interest had shifted from mathematics to ecology. 'Survivre' was his great pre-occupation. I actually joined him at a meeting of 'Survivre'. Afterwards I told him that I got the impression that a majority of the participants did not share his idealism and they were only struck by his celebrity status. As to be expected he strongly disagreed. I also pressed him about mathematics. He claimed that he was still interested but there were far more important things to so."

Such as surviving?

"Yes. He was very pessimistic. If the world continued the way it did, there would be a time, soon in fact, when it among other things would be impossible to do mathematics."

He had some major ecological disaster in mind?

"Obviously."

But he was not right this time?

"Depends on what you mean by 'soon'."

Did your relations end at this point?

"No they did not, although we would never meet again, we did keep up a correspondence."

A frequent one? And on what did you correspond?

"I would not say it was frequent. Sometimes a lapse of a year would occur between letters. While initially our correspondence had always been on mathematics, after 1971 this stopped and we confined ourselves to write about commonplace things."

So it was a correspondence between friends not colleagues?

"Yes. I did once breach a mathematical topic after 1971. I had sent him a reprint of a paper I had written on the motive of a surface and dedicated to him. I also asked him a few questions about motives. He acknowledged the paper as a nice one, but as to my questions he simply wrote that he had not thought of such questions for a long time."

So there was nothing controversial about your late correspondence?

"No, with one exception, which led to a minor crisis. He had sent me his *Récoltes et Semailles*..."

... which he wrote already in the 80's but whose existence did not become more widely known until later.

"This is true. I read parts of the manuscript, which was painful enough, not the whole thing, that would have been impossible for me. It was painful not only because I had problems reading it in French, but more to the point because I disagreed with him on so many points. The matter being delicate I chose to respond only superficially. He was very disappointed by my response. I realized how depressed he must have been while writing it, and I wrote back that although I could not agree with many points, I had not behaved like a friend and regretted it very much. He accepted my apology and after that our relations resumed to normal."

But not indefinitely.

"That is true. My last letter to him was dispatched in 1991. It was returned to me stamped 'undeliverable'. After that I completely lost contact with him."

But it was not personal?

"Not in the sense, that as I subsequently learned, this was the case with all his former friends and colleagues, and that he became a recluse in the Pyrenees. On the other hand how can you experience it as not personal?"

What is your lasting impression of Grothendieck?

"Of course I admire him as being one of the greatest mathematicians of the twentieth century. But I also admire him for his human qualities."

Such as?

"His honesty and his principled stands, against the military and for the poor and the weak."

You do not find him naive in some of his stands?

"Of course he was naive. 'Improving the world' is very different from doing mathematics. But nevertheless I admire his principled stands, his refusal to compromise his convictions. His anxiety for the future of mankind was sincere, and, I am afraid, justified as well. It must have frustrated him and hurt him deeply that his mathematical friends and colleagues did not follow him and share his concerns and worries. He did not compromise, also not when it came to himself and his life. He

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was logically consistent, not only in mathematics, and he accepted the consequences of it, also when it affected his personal life. This is what made his life so tragic in the end."

But this is not the way you prefer to remember him?

"No, it is not. I want to, and I actually do, remember him as he was when we met in Paris and Bures. He was the genius of course, but also generous and helpful as well as being cheerful and optimistic. This is the image that endures in my mind and I find myself truly privileged to have not only to have met him but to have known him."